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a. G. Prevost

AN INQUIRY
INTO THE CAUSES
OF THE
RISE AND FALL OF THE LAKES,

EMBRACING AN ACCOUNT

OF THE

FLOODS AND EBBS OF LAKE ONTARIO

AS DETERMINED BY A LONG SERIES OF

ACTUAL OBSERVATIONS,

AND AN EXAMINATION OF THE VARIOUS OPINIONS IN REGARD TO THE

LATE UNPRECEDENTED FLOOD

THROUGHOUT THE CHAIN OF GREAT LAKES.

TO WHICH IS ANNEXED

A LETTER TO DR. H. H. SHERWOOD ON HIS THEORY

OF

MAGNETISM.

BY EDWARD GIDDINS.

LOCKPORT, N. Y.
PRINTED AT THE COURIER OFFICE.
1838.

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TO THE PUBLIC.

The author feels truly grateful for the liberal patronage bestowed by subscription upon this production, and regrets exceedingly that it could not have been written under less disadvantageous circumstances, and thereby rendered more worthy general approbation.

The small portion of time his regular occupation allowed him to devote to it, and that cut up as it was, into irregular, inconvenient, and extremely short intervals, must plead in extenuation for its desultory and unconnected character, and for the many deficiencies and imperfections the critical reader will detect.

It is submitted with due respect
by the public's humble servant,
THE AUTHOR.

INQUIRY, &c.

The recent unprecedented rise of the waters in the chain of great lakes has excited much inquiry and speculation. The fluctuations of the lakes formerly attracted but little notice, being deemed scarcely worth a thought; but since the rise of water has began to affect many, residing on the lake borders, in a pecuniary point of view, the subject has grown to one of much interest, not only to such as are thus affected, but to every one who has a scientific turn of mind. Much has been said and written, and many theories advanced on the subject; but, unconnected with facts, and contradictory withal, they have proven of but little value to mankind or to science. Such being the case, it may be acceptable to state what facts I have in my possession touching the subject, so far as regards Lake Ontario. I have also given my opinion with regard to the causes, and spoken without reserve on the opinions of others; and if I have not been fortunate enough to hit upon the true ones, I shall at least have the satisfaction of knowing that I have not varied from them more than other theorists have varied from one another. This part of the subject, however, is distinct from the facts, and will pass for what it is worth. I present it with due deference.

From 1815 to 1827 I resided in the immediate adjacency of Lake Ontario, and had constant opportunity of observing its fluctuations. The phenomenon, in a scientific point of view, was not without interest, at least to me, and I was at considerable pains to take and record observations, especially during the latter part of this period. After making a few remarks upon some of the strange notions entertained by individuals upon this subject, I will state the result of my observations.

There is a tradition, relinquished with reluctance by many, and still adhered to by some, in spite of fact, that the lake has a regular septennial flood, produced by something independent of the weather —by some astronomical or occult cause, which they do not pretend to define. Every year brings new proof of its falsity, and common sense sees its repugnance to science, without shaking the faith of its adherents; for what have such to do with science or with fact? This tradition, however, worthless as it may be in itself, has had the effect, in connexion with the recent extraordinary rise of water, of exciting inquiry and speculation upon the subject, which otherwise might have remained latent; and many have ventured out with theories as variant from each other as some of them are from common sense. One of these theorists, who appears to be a strenuous believer in Dr. Sherwood's Theory of Terrestrial Magnetism, builds his theory of the lake floods upon that, and ascribes the late excessive flood on Lake Erie to the proximity of the magnetic pole! This, I think, is going a little farther than the Doctor, with all his apparent sanguineness, would venture himself. But, by the by, would it not be well to show the correctness, or at least consistency, of one theory, before we attempt to build another upon it? But I digress. This is no place to discuss Dr. Sherwood's theory of magnetism. Another place and time are reserved for this purpose.

Another writer states that a "large river" which formerly discharged its waters into Hudson's Bay, has been from year to year impeded in its course by floodwood, till finally it has cut a new channel into some one of the great tributaries of Lake Superior; and to this fact he ascribes the recent swell of the lake waters. Such an occurence would be a very extraordinary one indeed, and require the most positive proof to give it credence. The dividing ridge between Hudson's Bay and Lake Superior is much nearer the latter than the former body of water; it is doubtful, therefore, whether there is an area sufficiently extensive or a country sufficiently mountainous to form a "large river" tributary to those which discharge their waters into this lake. This ridge furnishes, of course, sources of streams which feed these two vast and distant bodies of water. Many of these sources, in their incipiency, without doubt, are situated near the summit, and a small obstruction in the rills and streamlets issuing directly from them, may incline their waters either down the northern or southern declivity, and thus render them finally tributary either to the Bay or the Lake. I have no particular knowledge of the physical geography of that section of country to assure me that such is the fact there, but such is and must be the fact in every place where streams have their sources near the summit of dividing ridges. We see it in several places on the ridge dividing the lake and Mississippi waters, and at more than one of these it is said canoes have passed with facility during high floods. It would therefore not be a miracle if some of the sources of the lake and Mississippi tributaries should mutually exchange directions and disemboguements. But who ever saw a "large river" thus circumstanced? Long before streams swell to this description, they form their beds, and, descending far down the declivities of the ridges which gave them source, receive numerous tributaries from the right and from the left; for them to return to their sources and pass the summit ridges, would not only require a suspension, but a reversion of the law of gravitation. As well might we expect to see the Susquehannah, after becoming a "large river" near Havre de Grasse, return to its source, pass the summit, and go down to the Ohio or to Lake Ontario; or the Amazon return to the Cordilleras, pass the summit of that vast ridge, and discharge its waters into the Pacific. The dividing ridges separating some of the extensive streams in European Russia are comparatively but little elevated above the sea, but who ever dreamed of throwing the Dwina into the Caspian or the Walga into the Baltic? The Red River was long choked up with floodwood, which the United States cleared away at great expense; but no one had the simplicity to apprehend a reversion of that stream either to the Rio del Norte or the Colorado of the West; yet such changes are scarcely more repugnant to the established laws of nature that that supported by our theorist.

From 1815 to 1821, my observations were not made with a regularity, frequency, or precision, to insure exactness as to the yearly rise and fall of the lake waters; and as set down in the annexed table, they may, therefore, occasionally vary, in a small degree from correctness; but from the attention I gave the subject, and the pains I took to make the table a near approximation to fact, I am confident the errors in no cases exceed three inches. From 1821 to 1827 my observations were made monthly, as near the first of each month as a settled state of the lakes would admit;—for a high wind of long continuance, blowing lengthwise the lake, will, near the Niagara, sometimes make a difference of nearly a foot above or below the mean level, as the wind happens to favor an accumulation or recession of the waters. These observations were recorded with minute exactness; and besides these, I took intermediate measurements whenever the change of level made it necessary; so that the maxima and minima for these years, as recorded in the table, may be depended upon as scrupulously exact. The table does not contain the monthly fluctuations, nor is it necessary—they are exceedingly small, and, compared with the yearly, are like the small waves that ride upon the heavy swells of the ocean. The principal flood, as will be seen by the table, attains its maximum in June or July,

From October 1827 to August 1838, a period of nearly eleven years, I took no measurements and made no observations whatever, as my removal from the lake rendered it inconvenient for me to attend longer to a subject in which, however, I took no little interest, and which I was in hopes of continuing without interruption.

On the 25th of August 1838, I again took a measurement, and ascertained from it, in connexion with the concurrent testimony of the neighboring residents and the water marks upon objects on the margin of the river, both the time and the quantity of the maximum for the present year, which may be found recorded in the table. The minimum, or extreme ebb may be found this fall or winter; and from the extraordinary scarcity of rain during the past four months, in connexion with the unexampled degree of heat, we may anticipate a fall of three feet at least; not so much, however, from excessive evaporation, although that has its share of influence, as from the absence of rain, and consequent draining of the streams which feed the lake tributaries and the lake itself.

I have been unable to learn any thing definite respecting the fluctuations of the lake between 1827 and 1838; but from all the information I can gather on the subject, worthy of credit, am of opinion that from 1828 to 1834, both inclusive, they did not materially differ from those of the preceding seven years; but since 1834, the floods have invariably exceeded the ebbs, a consequent yearly rise of the waters has ensued, until the present summer has found them higher than they have before been known for fifty years, according to the testimony of an old and respectable gentleman, who has resided during that period on the lake border. The highest floods, during the period of my residence at the lake, were those of 1815 and 1817, but by the table it will be seen that that of 1838 far exceeded either of them; being 22 inches above that of 1815, and 25 above that of 1817. I also learn that the floods reached their maxima about midsummer, as was almost invariably the case during the years of my observations.

There are some other facts connected with this subject which ought not, perhaps, to be passed entirely unnoticed: The yearly rise of water is almost invariably more rapid than the fall. When the yearly floods attain their height, they do not remain stationary there, but commence ebbing immediately. Not so with the ebbs, they are often stationary for months. The whole amount of water taken up from the earth by evaporation is manifestly equal to that of rain and snow which falls upon the same, abating from the former, the small quantity equal to that thought by some to rise from the ocean in the form of springs upon the principle of capillary attraction; otherwise, the humidity of the atmosphere would increase or diminish, as one or the other of these operations preponderated, until it attained a state of complete succity or saturation. The amount of evaporation from the whole ocean, must exceed that of rain and snow which falls upon the same, by the whole amount of water discharged into it by all the rivers and other streams on the globe, which, by estimation, is thirty-six cubic miles per day. The amount of evaporation from the land and inland waters, must therefore be less than that of the fall of rain and snow upon the same, by the same amount of thirty-six cubic miles per day, in order to maintain the streams in their present magnitude. Destroy this equilibrium of action and the streams and other bodies of water will swell or diminish accordingly. The extent of country which drains its waters into Lake Ontario, and which may be appropriately termed its valley, cannot be estimated at less than three hundred thousand square miles, while the surface of the lake itself does not exceed a fortieth of this amount. Should the amount of evaporation from this extensive area equal the amount of rain and snow which falls upon the same, the streams would all soon be dried up or drained off; the Niagara and the St. Lawrence would cease to flow, and Lake Ontario fall to the depth of the latter, and become a stagnant pool without an outlet. In order, therefore, to maintain the waters at their present level, the rain and snow must exceed the evaporation by the whole amount of water discharged at the outlet. The excess, therefore, of the former over the latter may be ascertained by an actual measurement of the outlet itself. Of these three particulars: the amount of rain and snow, the amount of evaporation, and the amount of the discharge at the outlet,—any two being given, the other may be found by simple subtraction or addition. The first may be known by the pluviometer, the second by the evaporator, and the third by measuring the current and section of the outlet.

In speaking of the general fact, that evaporation is less than the fall of rain and snow on land and inland waters, there are some exceptions which justice to the subject obliges me to notice, and which peculiar circumstances produce and make manifest: The Caspian and its neighboring waters of the same class, have no outlet, although many inlets, among which may be reckoned the majestic Walga; and that they do not communicate with the ocean subterraneously, is shown from the fact of their being many feet lower than either the Black sea or the Baltic. Evaporation, therefore, must exceed the amount of rain upon the surface of those waters, by the whole quantity received by the numerous inlets. The Mediterranean, in some respects, is thus circumstanced; a perpetual current is setting into it through the Straits of Gibralter, and it has numerous inlets, yet maintains its accustomed level. We can account for this in no other way than by an evaporation equal to the whole amount of rain which falls upon its surface, together with all the inlets and the current at the straits.

It is the opinion of some that the principal cause of the lake fluctuations is the excess or deficiency of evaporation, and among these are gentlemen of eminence in science, for whose opinions I entertain the most profound respect, and to dissent from which, is by no means pleasant or agreeable. Facts, however, will not warrant us in such an opinion; and I think it may be shown that evaporation, although an important element in the process, is by no means the principal one. Evaporation, in a great degree, is governed by temperature. All other things being equal, it increases in a geometrical, while temperature increases in an arithmetical, ratio; and although there are several circumstances to modify, in some degree, this law of increase, yet it is a well established fact that evaporation in warm weather exceeds that in cold. If the above quoted opinion, then, were well founded—if evaporation were the principal element, the lake would invariably begin to ebb in the spring, at the rise of temperature, be at the lowest in the heat of summer, and rise to its highest flood in winter when the depression of temperature diminished the amount of evaporation. For, evaporation, taking hold of the tributaries and the surface of the lake itself, simultaneously, would begin to show its effects *immediately* on each. But the exact contrary is the fact. The lake continues to rise till midsummer, and is lowest in the coldest season. Many who have not examined this subject thoroughly, are apt to imagine that the amount of evaporation differs widely in different years, and also in different seasons of the same year; but I think facts may be adduced, in connexion with the best established theories on the subject, to show satisfactorily that so far as regards the seasons, the difference has been much exaggerated, and as regards different years, evaporation approaches very near a state of equability.

The theories of evaporation advanced by the most eminent philosophers, are in conflict. The subject is an intricate one, and is less thoroughly understood than almost any other branch of natural philosophy. No wonder that this should be the case. It seems at first view but little short of mystery, that water should rise into the bosom of the atmosphere, in direct opposition to the law of gravitation, and after floating there for thousands of miles, precipitate and return to the earth again in obedience to the same law it had previously violated. The theory of Le Roy, supported by Dr. Black and Berthollet, supposes the aqueous vapour raised by a mutual attraction between it and the air, and the quantity proportionate to the degree of temperature and atmospheric succity. Saussure supposes the aqueous vapor raised by caloric alone, and then to combine chemically with the atmosphere. Dalton supposed it to be raised in a similar manner, but to exist afterwards in the air in a distinct state, supporting itself by its own elasticity; the quantity depending on the temperature, and the pressure of the vapor already formed. This theory, with some slight alterations, is now the prevailing one, and has obtained almost universal credence among philosophers. But let the cause of evaporation be what it may, there are some well established facts in regard to it, to which it will be necessary to advert.

The amount of evaporation from a given area in a given time, depends upon the weight, temperature, motion, and degree of humidity of the atmosphere. During excessive evaporation a reduction of temperature takes place in consequence of the increased capacity in the vapor for heat; and thus, strange as it may appear, one of the principal causes of excessive evaporation is checked and weakened by its own effect! In our present inquiry, the weight and humidity of the atmosphere may be disregarded; the first, because its influence is but trifling; and the second, because its state of incessant vacillancy renders it difficult to make an approximate estimate of its influence for any particular portion of time, in calculations in which it enters as a function. But little doubt, however, exists of its being about the same one year as another. There remains, then, but two particulars for consideration, viz: the temperature of the atmosphere, and its motion, called wind.

It is a well established fact in natural philosophy, that the force of aqueous vapor at any given degree of heat, indicates the quantity of vapor or evaporation which may be formed. Mr. Dalton, after many experiments and calculations, discovered the true relation of the force of aqueous vapor, and consequently of the quantity of evaporation, to temperature, all other attendant circumstances being equal. This relation he has tabulated, and the following is an abstract of his table; wherein the force of vapor is given in hundredths of an inch of mercury in the barometrical tube, for each tenth degree of temperature from 20 to 80, Fahrenheit's thermometer. The upper line gives the temperature, and the lower one the corresponding force of vapor:

Temperature,	20	30	40	50	60	70	80
Force of vapor,	13	19	26	38	52	72	100

After examining a large number of meteorological tables kept for series of years, in various parts of the United States, in the Canadas, and in Europe, and finding a close agreement in their general features, I selected one kept by Dr. Holyoke, in Salem, Massachusetts, not only because it contained the largest series of years, but because its location was such as to give a mean temperature not materially different from the medium of the whole lake valley. This similarity of temperature, however, is of but small consequence, as my present object is, not to show the temperature for any particular year, but rather its similarity for different years. Dr. Holyoke commenced his observations in 1786, and continued them without interruption, to 1828, when he died at the advanced age of 101 years. These observations are said to have been taken with great care; and copious extracts were published in the "Transactions of the American Academy." These extracts were condensed and published in the American Almanac; from which work the following extracts, in substance, are made. The figures represent the degrees of mean temperature for the coldest and hottest year, and for the coldest and hottest seasons, for the whole series of years from 1786 to 1828.

	Year.	*Spring.*	*Summer.*	*Autumn.*	*Winter.*
Hottest,	51	50	73	54	34
Coldest,	44	41	66	48	23
Difference,	7	9	7	6	11

By comparing this table of temperature with the preceding one of temperature and force of vapor, it will appear that so far as temperature is concerned, the comparative amount of evaporation for the coldest and hottest of a long series of years, is as the numbers 31 and 38, nearly; the same for the coldest and hottest of a long series of springs is as the numbers 7 and 10; the same for the summers, 16 and 20; the same for the autumns, 9 and 11; and for the winters, 4 and 5; excluding fractions. Now, supposing the evaporation for the coldest year in the series to have been 31 inches, which for the whole lake valley, is probably above the truth,* we derive the following result in inches, rejecting fractions, for the amount of evaporation for the coldest and hottest year, and also for the coldest and hottest spring, summer, autumn, and winter, in the whole series of years from 1786 to 1828:

	Year.	*Spring.*	*Summer.*	*Autumn.*	*Winter.*
Hottest,	38	10	20	11	5
Coldest,	31	7	16	9	4
Difference,	7	3	4	2	1

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*See note on page 19.

From this we find a difference of only 7 inches between the amount of evaporation for the coldest and hottest of a long series of years, and for the different seasons as in the table. Hence we may safely infer that the difference of evaporation between any two consecutive years would seldom exceed three inches. What now becomes of the force of the oft-told story of the excessive evaporation of a hot year, a hot summer, &c., compared with that of a cold one?

The last four months have averaged a degree of temperature higher, perhaps, than ever known here before for the same length of time. The mean for June was 69, July 73, August 69, and Sept. 62, giving a general mean for the four months, of 68. From the above tables and estimate of yearly evaporation, we derive for the evaporation of June last, 6 inches; July, 7; August 6; Sept. 4; and for the four months, 23; but as the atmosphere was remarkably dry, it may have somewhat exceeded this. From the middle of July to the present time (Oct. 1st) I am informed the lake has fallen about twenty inches; granting the amount of evaporation, then, as deduced from the above statement, to be 13 1-2 inches during that time, we are led to the conclusion that the amount of discharge at the outlet was greater than that received by inlets and by rain on the surface, by a quantity sufficient to raise or depress the surface six and a half inches.

Again, if evaporation really has that supreme influence which many claim for it, how does it happen that the Ohio and its tributaries are so remarkably low, while the lake is upon the other extreme? The drought has prevailed in the lake valley, at least in the south part of it, as severely as in that of the Ohio; the lake waters are much more exposed to the operation of evaporation, open as they are to the free access of the sun and full influence of the wind, than those of the Ohio and its tributaries, passing as they do, through dense forests, where the sun's rays are excluded, the wind's influence is lessened, and the increased humidity checks the ascension of vapor. These effects are to be attributed to a deficiency of rain as the principal cause, and to evaporation as an auxiliary; and these two antagonist agents always act so far in concert that an excess of one ever accompanies a deficiency of the other. The sources are *drained off* for want of replenishment, and the streams fall as a necessary consequence. The valley of that river being small in comparison with that of the lake, the effects of the drought are proportionably sooner. The probability is (if the drought has extended through that region) that Lake Superior has fallen more in proportion to its rise, and much sooner than Lake Ontario; for its valley being much smaller, it presents the effects of drought or flood in a comparatively shorter time. Ontario, being the lowest link in the chain of great lakes, and consequently having the most extensive valley, will be the last to exhibit the effects of the drought or flood, but when exhibited they will be proportionably augmented.

Within and near the tropics, instead of winter and summer, fall and spring, snow and drought, there exists, properly, but two seasons,—the wet and the dry—the season of rain and the season of drought, as they have been termed,—and the streams, as a consequence, are subject to periodical swells. The overflow of the Nile is well known to be occasioned by the excessive rains of the wet season which fall in the interior of Africa. But the overflow takes place in Lower Egypt, long after, where rain never falls; it was therefore, for ages viewed by the inhabitants as an inscrutible mystery, and would have been to this day, had the Egyptians continued to reason as some of us at the present day are wont to do.

It is a common expression, when streams are low, to say "their sources have *dried up;*" inferring that evaporation was the sole agent in their failure. Although evaporation undoubtedly has an influence, still it does not seem consonant to fact that it is the most prominent actor; and it would in such cases be nearer the language of philosophy, perhaps, to say "their sources have *drained off.*" In times of great drought (and they occur in the cold season as well as in the warm) springs fail,—except those scarce and singular ones which always maintain their steady unchangeable flow, and which, from this circumstance, are supposed to be fed from the ocean by capillary attraction; wells of water fail also, which flow many feet beneath the surface, where evaporation never operates. The subterranean streams which feed springs and wells, are supplied by rains and melted snow; which, absorbed by the earth on the mountain acclivities, burst out again in springs below, or furnish artificial wells from their deep and hidden beds. When these cease to receive their accustomed support and nourishment, they fail of course. It would seem, then, that by far the most prominent cause of the *fall* of our lakes and streams, is the *deficiency* of rain and snow, rather than the *excess* of evaporation.

The effects of the wind, which remain to be considered, will be found to further modify, and in a great degree, the difference in quantity between summer and winter evaporation. A high wind is acknowledged by all writers on the subject, to greatly accelerate it. Some are of opinion that it increases its quantity from five to ten and even to twelve fold. Indeed, experiments have proven it. And what is more rational? The quantity of air which comes in contact with a given surface in a given time, is in the ratio of its velocity. A wind of 12 miles an hour will bring four times the number of particles in contact, as one of three miles, and the evaporation, *cœteris paribus*, is four times as much. In order to form an estimate of the effects of different winds upon evaporation, we give their velocities as determined by the anemometer:

Gentle breeze,	4	miles an hour.
Pleasant do.	8	" " "
Brisk gale,	16	" " "
High wind.	36	" " "
Storm,	62	" " "
Hurricane,	88	" " "
Very violent hurricane,	100 to 120	" "

But this is not all: the contact, in a high wind, is more violent, and therefore more perfect; and the surface of the water also becomes increased in area by the commotion. Now, it is well known that we have our highest winds in the cold season,—the summer being comparatively calm to the winter, fall and spring. This fact, then, it would seem, tends to counteract the effects of temperature, and to an equalization, of opposing causes. The strong northwest and west winds, so prevalent during the cold season, are the most powerful evaporators, perhaps, that act upon the lake valley.

Some, whose opinions are entitled to respect, refer the lake floods to excessive rains, and rest satisfied with their hypothesis without tracing it far enough to discover its inconsistency with facts. The lake valley is not situated in a climate where excessive rains prevail; and when they do occur, they are too limited in extent—they spread over too small a portion of this vast area to produce any considerable rise of the lake waters. A rainy season undoubtedly tends to retard the lake ebbs, and maintain the waters above any extraordinary depression; but that rain is not the principal cause of the lake floods may be made manifest. If we have any one season more rainy than the others, it is autumn; upon the above hypothesis, then, the lake would begin to rise in autumn and reach the acme of its flood in winter. But the exact contrary is the fact—the lake is lowest in winter and highest in midsummer, and this in opposition to the concurrent effects both of increased rain and diminished evaporation at the approach of cold weather. Both these causes, then, acting in concert to elevate the lake in winter and depress it in summer, without actually producing that effect, must be neutralized by some counteracting cause which facts show to be far more powerful than their combined influence. This cause it is our object to find out. But as every detection of error is one step towards the truth, the examination and refutation of false theories may not be altogether profitless; and we will now take up one of quite a different character from any of the preceding:

There is a class of theorists who attribute the lake floods principally to the dilatation of the water in the hot season, occasioned by the augmentation of caloric at that time within its bosom; and as proof, they adduce the fact that the lake is invariably the highest in summer. There is a plausibility in this,—and it will be necessary to occupy a small space in its confutation. The water of Lake Ontario is at its highest temperature about the first of September, when its surface is about 60 degrees Fahrenheit. In winter the uniform temperature is about 40. At the depth of 150 feet, at the furthest, the temperature is nearly invariable through the year, and in the neighborhood of 40. I give these estimates solely from judgment, and should they be found to differ a few degrees from fact, the consequent error in the result of the following calculation will be small and unworthy notice. From the depth of 150 feet to the surface, the temperature, on the first of September, gradually increases from 40 to 60. This 150 feet of water, then, may be considered as having a mean temperature, at that time, of about 50, and in winter, of 40. Let us now see what will be the amount of expansion, from the temperature of 40 to that of 50. Philosophers have taken unwearied pains to determine the expansion of water by heat; and the labors of Lussac, Laplace, Dalton. Biot, Huoy, and many others, show that its amount from 32 to 212, or rather from 40, the maximum density, is about a twenty-third part of its volume. Equal increments of heat, however, do not produce equal increments of expansion. Far from it. Mr. Dalton found the expansion to be nearly as the square of the temperature, reckoning from the maximum of density. From this we find the expansion from 40 to 50 to be about an eight hundredth part. This amount of expansion in a body of water 150 feet deep, would raise the surface a small fraction over two inches; and this is the greatest possible effect of caloric in swelling the waters of Lake Ontario.

There are some who attribute the extraordinary rise of the lake, of late years, to a change that is taking place in our seasons, and this change they refer to astronomical causes, slow but regular in their operations, and destined to produce the most disastrous effects.

I am not aware of any astronomical cause that would produce such a result. Any considerable change in the position of the earth's axis, with respect to its surface, whether the effect of a sudden impulse, or the slow and gradual operation of millions of ages, would reverse, in a great measure, the relative position of the land and water; continents would appear where now rolls the ocean wave, and our most elevated mountains would be swallowed up in the deep; and should such a revolution take place suddenly, no one could expect to escape the general devastation. But we have not the shadow of evidence that such a change has ever taken place, and there is no probability that it ever will. Nothing could produce it but a collision with some vast body; the change would then consequently be sudden, tremendous, and all-devastating; and the new position would depend upon the force and obliquity of the impact.

There are some facts in astronomy which may have a bearing upon this subject. The obliquity of the ecliptic is known to cause the seasons. It diminishes at the rate of about 52 seconds of a degree in a century; and in the same ratio our seasons will probably approach an equalization. Admitting this diminution to continue, (which, however, will not be the case, as the obliquity is periodically increasing and decreasing,) the seasons would be completely equalized in about 162,000 years; when winter and summer would cease to be known, and a perpetual spring prevail. This, however, would not be accompanied with any change in the mean temperature of the earth, as it supposes no change in the sun's distance; its effects upon the water would therefore probably be no more than what the diminished range of temperature would produce. There is another astronomical period, however, of much shorter completion, which acts periodically in concert and in opposition to this, and also actually produces a periodical change in the mean temperature of the earth, and periodically increases and diminishes the difference of seasons. The earth revolves about the sun in an elipse, in one of the foci of which the sun is placed. It must, therefore, during every annual revolution, be at its greatest and least distance from the sun, or in its aphelion and perihelium. The difference of these two distances is about three millions of miles; the sun, therefore, is that much nearer to us in the one case than in the other. The earth is now in aphelion about the 1st of July, and in perihelium about the 30th of December; we are therefore about three millions of miles nearer the sun in winter than in the summer. This position of things undoubtedly tends to mitigate the rigor of our present winters and the heat of our summers—or in other words to an equalization of the seasons, in the northern hemisphere, and to the opposite effect in the southern.

If the earth returned to perihelium every tropical year, exactly, or in other words, if the line of the apsides was stationary with regard to the tropics, the anomalistic and tropical years would become of equal length, and this order of things would be perpetual. But the tropics receding about 20 minutes yearly, while the line of the apsides advance about 5, there results from the combined influence of these opposite motions, a difference of about 25 minutes between the length of the tropical and anomalistic years; as a consequence, the sun goes into perihelium that much sooner every year, which in about 10,500 years amounts to six months; and therefore at the end of this period the places of aphelion and perihelium will be reversed—the sun will be nearest in summer and furthest off in winter; the difference of seasons will be greater—the winters colder and the summers hotter by about one thirtieth part, with us, but the contrary in the southern hemisphere. In double this period, (21,000 years) the apsides return to their present position, mitigating the vicissitudes of the seasons as they do at present. This phenomenon also probably has some influence upon the water, but it is undoubtedly small, and so slow and tardy withal, that many ages would be necessary to render it sensible. Those, therefore, who have theorized themselves into apprehension of evil from such causes, would do well to banish their ill grounded fears, recall their speculative spirits from so vast a dip into futurity, and exercise their investigating powers upon changes less tardy.

What, then, it may be asked, is the cause of the Lake fluctuations? I answer, no one exclusively. The evaporation, humidity, weight, temperature, and commotion of the atmosphere, may all in justice claim a share in their production. But their combined influence, even were it possible for them to act in concert, would not, as has, I presume, been satisfactorily shown, account for the yearly floods of the lakes. They, however, never act in concert,—but, ever varying and ever more or less opposing or counteracting each others movements, their combined effects must ever be irregular, inconsiderable, and frequently evanescent. There must, therefore, be some predominant cause, comparatively regular in its operations, and altogether more powerful than the combined influence of others, to produce the yearly lake floods. What is it? SNOW.

The yearly rise of the lake will ever be found to bear an approximate ratio to the average quantity of snow throughout its extensive valley. I do not say an exact ratio, because there are many disturbing causes which have more or less influence, according to the amount of coincidence or opposition of their action. The time occupied in the conversion of the snow to water, is an important element in the calculation of its effects. The more sudden the more effective. The manner of its conversion is also worthy of notice. Whether it be by heat, by rain, or by both combined. Whether it take place simultaneously through a greater or less extent of the lake valley, &c. &c. It is probable that the average deph of snow throughought the whole 300,000 miles of the lake valley, is sometimes not less than twenty-four inches, at the commencement of its conversion into water at the breaking up of winter; this, in its solid settled state, is equivalent to about four inches of water throughout this whole vast extent. Were it possible to deposite the whole at the same time in Lake Ontario, it would raise its surface not less than thirteen or fourteen feet. Should the time of convertion be protracted to one, two, or three months, accompanied with rains sufficient to supply the outlet, the effect would not be diminished. The average amount of snow in the lake valley, varies in different winters between very wide extremes; and the yearly rise of water, due allowance being made for the disturbing causes above named, will be found to vary accordingly. If this theory be correct, the upper lakes will soonest exhibit the effect of spring thaws and summer droughts; because their valleys being comparatively small, require less time to spread the effects of change throughout. Lake Ontario, whose valley embraces those of all the other lakes in the chain, will be latest in exhibiting either. And so it is in fact.

The causes of the lake fluctuations are too manifest to raise a shadow of doubt. They are the ascent and descent of water,—evaporation, rain and snow; but the proportion of influence exerted by each of these is the true question at issue, and must remain undecided, or at least involved in more or less doubt, until Meteorology is better understood than it is at present. It is astonishing that a science, a thorough knowledge of which would be of such incalculable benefit to mankind, is so shamefully neglected. If there is any one branch of useful knowledge having higher claims to the encouragement and pecuniary aid of government than others, it is METEOROLOGY. Individual means can do but little. A long series of close observations, taken at a multitude of points with care and skill, must be effected before any thing really valuable can be developed. But this would require pecuniary means which individuals or companies are not able to sustain. It must be done by government—and we have hopes that it will be. Creditable, indeed, would it be for our country, were our statesmen in all cases qualified to distinguish true science from quackery;—that they are not, we have a recent proof that will long be a reproach to the scientific reputation of America. But this has reference to another subject to be discussed hereafter.

Last May Congress was memorialized on the subject of Meteorology, by J. P. Espy and the Philadelphia Lyceum; and it is anxiously hoped, both for the invaluable benefits that must result, and for the scientific character of our country, that the aid asked for will be promptly granted. Mr. Espy has pursued the subject with a zeal and ability that few possess; and the discoveries he has already made, are of an important character. There is not a man of scientific attainments on earth, but would give his sanction to the plan of simultaneous observations proposed by Mr. Espy. The utility of a thorough knowledge of Meteorology, to the agriculturalist, the mariner, the merchant, the philosopher, and to mankind generally, cannot be denied. There is not, in all probability, a complete set of Meteorological observations taken in all America! This is in some degree owing to the close attention, and the multiplicity of instruments necessary for the purpose. Competent individuals without doubt would volunteer in sufficient numbers, to make the observations without money and without price, could they be furnished with the necessary instruments. There is an unaccountable apathy on this subject pervading the country; much exertion will be required to shake it off. The importance of the science—the benefits that must flow from a thorough knowledge of its principles, must be explained and spread before the public, and its connexion with the other sciences made known. Light, Heat, Electricity, and Magnetism, are inseparably connected with it, and will be accelerated or retarded in their advancement in a corresponding ratio with that of their neglected companion.

We are all of us anxious enough to know the future changes of the weather,—but should we ever attain this desirable knowledge, it will be through a thorough knowledge of Meteorology, and this through a long series of observations, made at numerous points, and then recorded, compared, and built into correct theory. The prediction of eclipses and other aspects of the celestial bodies could never have been effected but as a subsequence of the closest observations on their volumes, figures, distances, velocities, and trajectories. On these observations, as a ground work, the celestial mechanics have been thoroughly elucidated by the mathematician—rules have been deduced by which their various phenomena may be predicted for any past or future time, with scrupulous exactness. Without the guide of observations, all the mathematics man is master of could have effected nothing on this point—reasoning *a priori* would forever have been ineffectual. As with Astronomy so with Meteorology,—observation must lay the ground work of knowledge.

A Meteorological observatory ought to be established in every Congressional District in the United States, and in the territories, and maintained and encouraged by the government,—with faithful, competent, industrious superintendents to take and register observations on the weight, humidity, temperature, and electrical phenomena of the atmosphere—the amount of rain, snow, hail, frost, dew, and evaporation—the proportional transparency or obscurity of the atmosphere—the class, motion, and changes of the clouds—the strength direction, and character of the winds—the rise and fall of streams and other bodies of water—the aurora borealis and other remarkable phenomena—the fluctuations in the intensity and direction of the magnetic force, and any other remarkable occurrence.

It will, perhaps, be expected that a word be said on the supposed rise of water in Lake Erie, occasioned by the Black Rock Dam. A mathematical solution to this question requires for data, the transverse section of the river at that point, the transverse section of the obstruction, the velocity, and, if great, the slope. These particulars having been obligingly furnished, enable me to state as follows, which may be depended upon as a near approximation to fact, viz: When the lake is very low, the rise of water occasioned by the Dam will not exceed four inches; when it is very high, it may amount to seven inches. And between these two extremes it will fluctuate with the stage of the lake. This is the maximum effect of the Black Rock Dam in raising the waters of Lake Erie. But I am infringing upon the province of the Engineer, and therefore drop the subject.

Lockport, October 1, 1838.

Note referred to from page 11.

Near London, where the mean yearly temperature is about 50 degrees, the evaporation has been ascertained to be about 2 feet, viz: 7 inches in the spring, 10 in summer, 5 in autumn, and 2 in winter, omitting fractions. So far as temperature is concerned, it must be considerably less than this in the lake valley.

TABLE

OF THE YEARLY FLOODS AND EBBS OF LAKE ONTARIO,

From the year 1815 to 1826, both inclusive, and for a part of each of the years 1827 and 1838; wherein the zero is a point five feet below the upper surface of the upper end of the capsill of the wharf at the mouth of the Niagara River, near Fort Niagara.

Year.	Lowest water.	Highest water.	Yearly range.	Time of highest water.	Water nearly stationary.	
					From.	To.
1815	32	64	32	July,		
1816	30	50	20	"	November.	March.
1817	38	61	23	June,	"	"
1818	32	54	22	"	December.	April.
1819	21	50	29	"	November.	March.
1820	15	38	23	July,	"	"
1821	20	58	38	"	December.	April.
1822	23	50	27	June,	"	March.
1823	24	54	30	" 20th	"	" 24th
1824	26	56	30	July 10th	"	" 1st.
1825	8	35	27	June 15th	"	"
1826	8	38	30	July 4th	"	"
1827		58		June 15th		
1838		86		July 15th		

The figures in the 2d colnmn of the above table represent the inches above zero where the lake water stood when at its extreme ebb or lowest point. The figures in the 3d column, in like manner, represent the inches above zero where the water stood when at its extreme flood or highest point. The 4th column contains the yearly range in inches, or the distance from extreme ebb to extreme flood. The 5th gives the time of extreme flood. The 6th and 7th give *from* and *to* what time the lake remained at its ebb, or was nearly stationary.

The ebb for 1827 was not taken, as the observations were discontinued on the 1st of October, when the water stood at 50, and was rapidly falling, the ebb therefore probably stood at about 30, which would give a range of 58 for that year.

On the 25th of August of the present year the water stood at 73, having fallen 13 inches from its extreme flood. On the 1st of October it stood at 66 and was rapidly falling; the extreme ebb can probably not be ascertained before the end of the year, and from present indications it will stand at about 46, which will give for the present year, a range of 40.

DR. SHERWOOD'S

THEORY OF MAGNETISM.

In June of the present year, 1838, Dr. Henry Hall Sherwood, of the city of New York, sent up a memorial to Congress, praying their aid to enable him to accomplish certain objects therein specified. This Memorial, in the House of Representatives, was referred to the Committee on Manufactures ;—in the Senate, it was referred to the Committee on Naval Affairs, who, on the 3d of July, submitted a Report, by their Chairman, N. P. Tallmadge, of which the following is the caption :

> "The Committee on Naval Affairs, to whom was referred the Memorial
> " of Henry Hall Sherwood of the City of New York, claiming to have
> " made new and important discoveries in Magnetism generally, and
> " more particularly in the Magnetism of the Earth ; and representing
> " that he is the inventor of an instrument called the Geometer, whereby,
> " without the aid of the quadrant, or sextant, or chronometer, and with-
> " out taking a celestial observation, it is practicable and easy, at sea and
> " on land, and in all weather, to determine, *merely by the dip of the needle*,
> " the variation of the needle and the latitude and longitude of any
> " place on the surface of the globe ;—and praying the aid of Congress
> " to enable him to proceed with the publication of a work explaining
> " these discoveries and the uses of that instrument, and to commence
> " the manufacture of the same in the most perfect manner."

The following is the first paragraph of the report :—

"That from the late period in the session when this Memorial was referred to the Committee, they have been unable to give to the subject that thorough examination and investigation which, under other circumstances, they would have been pleased to give. They have, however availed themselves of the opinions of scientific gentlemen, who have had a better opportunity of examining it than the limited time of the Committee has permitted to them. From their opinions, as well as from their own examination, *the Committee are fully persuaded that the discoveries and invention of Dr. Sherwood are entitled to the most serious consideration of the public, and to the encouragement and patronage of Congress. The Committee regard them as highly interesting and important to the navigation and commerce of the United States, and as bidding fair to open a new era in the history of the science of Magnetism.* They refer to the communication of Dr. Dwight as containing a full and ample view of the discoveries of Dr. Sherwood, and to those of other scientific gentlemen and others of the Navy, as to the principles and practical operations of the Geometer."

This Report was ordered to be printed, and that five thousand additional copies be printed for the use of the Senate.

Notwithstanding the very favorable opinion entertained by the Committee, of Dr. Sherwood's discoveries and invention, and the high encomiums passed upon them by several of the "scientific gentlemen" whose communications are appended to the report, I feel myself warranted in saying that any man versed in the science of Magnetism, or possessed of sufficient mathematical knowledge to solve a spherical triangle, would pronounce them puerile in the extreme, and full of absurdity, with the exception of the peculiar manner in which the Doctor professes to have permanently magnetized a circular plate and ring; these particulars, so far as my knowledge in the science of magnetism extends, are new, and original with Dr. Sherwood; and, as the magnetization of the ring in the manner he states would go far towards upsetting a theory which is rapidly coming into favor, it is so far important. But when we look at this pretended discovery, connected, as it is, with so much that is self-contradictory, and so much, I am sorry to say, that looks like imposture, a heavy doubt rests upon it, which time alone can dispel.

Such being the character of these pretended discoveries, they would not have been deemed worthy this notice, were it not for the lofty position they have assumed with regard to the credit of our country. It was but a mere accident that the aid asked for by the memorialist was not granted. What a national disgrace would have been the result! I must add that the extreme anxiety manifested, both in the memorial and in the communication of Dr. Dwight, that the aid be *immediate*, looks as though it covered some sinister design.

The following Letter will give a more copious view of the subject:

A LETTER ADDRESSED TO DR. H. H. SHERWOOD, ON HIS THEORY OF TERRESTRIAL MAGNETISM.

Lockport, N. Y., Nov. 1, 1838.

Sir.—In addressing you this letter, I beg leave to assure you that it was dictated through no unfriendly feeling towards you or any of the supporters of your theory; and I refer to your Card of the 21st of August last, as my sufficient apology for the task I have undertaken. You there give a

general invitation to the scientific of our country, to investigate your Theory of Terrestrial Magnetism, and to state, under their own proper signatures, their own objections to it, as well as those they derive from books and treatises on the subject. My communication on the subject of your discoveries, published in the Niagara Courier of July 25, was written previous to my having seen your memorial to Congress, or the report of the Senate thereon, or any thing else on the subject except an extract from the report, published in a New York paper. There is nothing, however, in that communication, that I would recall; but a previous examination of the memorial or report would have rendered it much more full and explicit. I will now endeavor to supply the deficiency. With respect to the magnetic apparatus you exhibited in this village last summer, I would observe that I shall not trust my memory to reason from the explanations you then gave of their principles and action, but shall prefer making use of the memorial and report for that purpose.

As the necessity, or even propriety of this letter, or of any other individual exertion on the subject, may be questioned by some, I would reply that the prominent position it has assumed, and the very general and intense interest it has excited, demand the exertions of all who regard the scientific character of our country, in behalf of a thorough investigation and prompt exposure of its merits or demerits. If, indeed, your theory should prove a true representation of nature, the name of "SHERWOOD" would surely and justly rank second to *none* on the list of fame, or of human benefactors. But, on the other hand, should it prove to be but puerility and quackery, it will at the same time prove that the scientific reputation of our country has been deeply and shamefully compromitted, and will place that same name in a far less enviable position. To save from obloquy, then, this reputation, an examination and exposure are demanded. The disposition of the Congressional document on the subject implies such examination, and you, yourself, sir, have invited it. There are thousands more competent to the task than myself, and one, eminent in science has partially performed it; but I have pledged myself in a way that I cannot in honor decline, however anxious I might be of doing so.

From your memorial I learn that all your spare time, for upwards of twenty years, has been devoted to the investigation of the science of magnetism, and to the study of the nature and properties of the magnet; and from the letter of Dr. Dwight, that for many years it has been the commanding business of your life. With such advantages, sir, it is to be presumed that you must be familiar with all the discoveries and experiments of any value in the science, and with all such inventions as are under the government of its principles. If, then, it should appear that you have infringed upon the rights of others, by claiming as your own, inventions or discoveries which justly belong to them, you cannot plead ignorance in extenuation of the plagiarism, with any prospect of success.

You claim, sir, in your memorial, to have made important discoveries, hitherto, as you believe, "unknown to mankind, which will render magnetism an exact science, and prove of great practical importance to mankind." One of these discoveries is, that you have magnetized a circular ring and circular plate of iron; a result which you say has hitherto been regarded as impossible.

The peculiar manner of magnetizing a ring and plate so that the poles shall be at or near the edges, and diametrically opposite, may be original with you—at least my reading is not extensive enough to deny it. But are you sure that your mode of magnetizing will always give the same result? Two pieces of iron or steel can rarely be found of exactly the same or of uniform texture, and their magnetism will accordingly vary. We see this in magnetizing bars and needles. The poles of which will sometimes have different intensities, and sometimes be situated at different distances from the extremeties; the polar axis will sometimes make a greater or less angle with the axis of the needle or bar; and the poles, moreover, never concentrate at single points. Besides, there are very often *consequent* points, as they are called, or intermediate poles, which derange the directive force, and render needles more or less faulty. I see not why the same discrepant results may not take place in your mode of magnetizing rings and plates; and if so, it would rarely happen that the two poles would be diametrically opposite. The least difference of texture or temper in any two opposite segments would, in all probability, throw the poles more or less from their mutual opposition, and from the axis traced on the ring or plate. And here, possibly, may have originated that strange error, that "the angle formed by the magnetic axis and the axis of the plate is invariably 23 degrees and 28 minutes." But the magnetization of circular rings and plates is by no means original with you; and your own quotations prove that you were not ignorant of its having been done by others. From treatises on the science of magnetism we learn that the north pole of a magnet placed perpendicularly upon the centre of a circular plate of iron, will give that plate a south magnetic pole in the centre, and a weak north pole all around the edge. If the plate be irregularly shaped, or have radii of different lengths, it will have a south magnetic pole in the centre, as before, and a north pole at or near the extremity of each radius. If the plate be star shaped, each point will possess a north pole; if in shape of a cross, the four ends; if of a triangle, the angular points; if of a straight bar, the two ends; while in each case the south pole will be found in the centre. Reverse the poles of the magnet, and exactly the reverse in each case will take place. In short, there is no end to the variety of shapes that may be given to magnets; but to fix their poles where we please, is quite a different task. "A circular ring, when magnetized," says Dr. Roget, "exhibits no magnetic properties as long as the ring is entire; but when broken into any number of portions, each portion has two poles, possessing all the properties of an ordinary magnet." Now, that this should and must be the result, has been mathematically demonstrated upon Ampere's hypothesis of magnetism. The establishment of the fact, then, that a ring can be so magnetized as to give the results you state, would overthrow Ampere's hypothesis.

You state that in magnetizing a ring or plate, the magnetic axis invariably makes an angle of 23 degrees and 28 minutes with the axis of the plate. It would be difficult to believe, had we not your positive assertion, that from your mode of magnetizing, you could determine this angle to the nearest minute. To do so, you must previously know the exact central point, or point of greatest intensity, of the pole of the magnet you make use of, carry a remarkably steady hand, and be blest with more than ordinary eyesight!

Admitting your theory of the dip, that it is invariably equal to the magnetic latitude,—strange inconsistencies will result, at variance alike with other parts of your hypothesis, with the well established fact, that the intensity of the magnetic force is inversely as the square of the distance, and with all possible laws of attraction and repulsion. The shape of the magnetic curves, formed by the combined influence of the two poles of a magnet, a rude resemblance of which may be seen by placing iron filings on a smooth surface over a magnet, is sufficient to have taught you the absurdity of this part of your theory. At the magnetic equator, the dip, by your hypothesis, is nothing. This is a legitimate result of two magnetic poles diametrically opposite, and of equal intensity. Remove the needle to the magnetic latitude of 30 degrees, and, by your hypothesis, the dip will be 30 degrees, and the needle will therefore point directly to the extremity of the magnetic axis. Now, admitting the magnetic poles to be situated at these extremities, the needle to have this dip, must be altogether influenced by the nearer pole—the least possible influence of the other would diminish the dip. But to show the inconsistency plainer, let the needle be removed to the magnetic latitude of 60 degrees; the dip will then be 60 degrees, and the direction of the needle will be to a point in the axis, very nearly half way from the surface to the centre of the sphere; a result that could not possibly take place unless the poles were situated still nearer that centre. Such a result, with poles near the extremities of the axis would prove a repulsive power between the further pole and the corresponding pole of the magnet, and an attractive power between the other two corresponding poles. To reconcile such anomalies must, indeed, require those "very high powers of comprehension, analysis, and construction," attributed to you by your friend Dr. Dwight.

Another discovery you profess to have made, and which you believe to have been "hitherto unknown to mankind," is, that the poles of the ring and plate are not on the outer edge, but within, and at a depression of 2 degrees and 35 minutes. The discovery of this particular angle of depression is undoubtedly original with you, and is of a similar character with that of the 23 degrees and 28 minutes before referred to. No sane man will ever attempt to rob you of it, and you need be under no apprehension of loosing any part of the honor due to such discoveries. Almost any work on magnetism will teach us the fact that the poles of magnets are not at their extremities. Dr. Roget, an author you, yourself quote, has considerable to say upon this fact, and upon its causes; and the celebrated Huoy gives an ingenious method of determining the exact distance from the extremities. This distance is not always the same, nor is it always proportional to the length of the magnet, but seems to be governed, in a great degree, by the texture of the magnet as well as by the manner of magnetising it. Huoy found that in a steel wire of 25 inches long, the poles were about ten lines from the extremities.

Your theory acknowedges but two magnetic poles, which are placed in the polar circles and antipodal to each other; yet its inconsistency is such that in solving questions by it, as many poles are to be recognized, differing in latitude as well as longitude, as there are problems to solve, as I shall by and by show. Your theory, recognizing but two magnetic poles, is at variance with the most careful observations of the most celebrated philosphers of the present day; and we learn from Dr. Roget, that the recent discoveries of Mr. Hansteen have completely established the fact of two north magnetic poles, and their respective locations in the neighborhood of Hudson's Bay and in Siberia; and that these poles differ in latitude and in intensity as well as in longitude. This discovery, with the probability of corresponding antarctic poles, will completely account for the well known irregular tortuous shape of the magnetic equator. Dr. Roget, in another place, speaking of the labors of Hansteen, observes, " We have every reason to expect that great light will be thrown on this department of the science, from the labors of Professor Hansteen, of Christiana, who is now travelling at the expense of the King of Sweden, and with the permission of the Emperor of Russia, for the purpose of observing the magnetic dip, variation, and intensity, over the whole of the north of Europe and Asia. He has particularly directed his attention to trace the course of isodynamic lines, or lines of equal intensity. Now, sir, for you to urge a crude and self-contradictory theory upon the world, in opposition to facts established by a multitude of observations, taken with great care and assiduity at various times and places, by philosophers of the greatest skill and eminence, betokens a degree of self-assurance incompatible with true science.

Another of the discoveries which you claim in your memorial, and which, I presume, no one will ever be inclined to rob you of, is, that the needle, on the magnetic equator, has no variation. Were this a fact, sir, the needle would exhibit the strange paradox of pointing, not to the magnetic pole, where the attractive influence exists, but to the terrestrial pole, which on some parts of the magnetic equator, lies, by your theory, in a direction 23 degrees and 28 minutes to the right or left of it, and on the other parts, in directions from this number of degrees to zero. Were this actually the fact, it would be an evidence of the coincidence of the magnetic pole with the terrestial pole, or at least with the terrestrial axis, or that the poles were not mutually antipodal. If your theory recognized the existence of two poles which were not antipodal to each other, then it would result that on many parts of the earth, and indeed on all parts which were not in a great circle cutting these two poles, the needle would point to neither pole, for, not being in the great circle passing through them, it could not point to both, and being at the same time influenced by both, it could point to neither.

Your discovery that the line of no variation is a true great circle of the earth, is so completely at variance with every known fact upon the subject, that it is unworthy of comment, connected, as it is, with an hypothesis so full of contradiction and absurdity.

Another discovery which you claim as your own, is, that the line of no variation is a magnetic meridian passing through the magnetic poles, but not through the poles of the earth. You say that it is that magnetic meridian which passes at the distance of 23 degrees and 28 minutes from the poles of the earth. Dr. Dwight says it is that which, after cutting the magnetic poles, passes at the distance of 6 degrees 28 minutes from the poles of the earth. In these two accounts there is a discrepance of 17 degrees,—one of them is probably a typographical error. No matter for this, as both are equally absurd and equally impossible upon your hypothesis of two antipodal poles. In order to show this absurdity in the plainest manner possible, let us suppose the needle placed at the magnetic equator upon the line of no variation, and then gradually moved upon that line towards the magnetic pole. As it approaches that pole, its direction will be more and more from it, and when it arrives in its immediate neighborhood, will lay nearly at right angles with it, as its direction is still, by the hypothesis, to the pole of the earth. The location given this line by Dr. Dwight, makes the paradox still greater, for by it, the needle, after passing the magnetic pole, and being still, by the hypothesis, directed to the pole of the earth, would point within 6 degrees and 28 minutes of directly *from* the magnetic pole—from the very point where the attractive influence has all along been supposed to be concentrated; or, in other words, the needle, on this part of the line of no variation, in being directed to the terrestrial poles, where there is nothing to influence it, would have a variation from the magnetic poles—from the concentrated points of attractive influence, of no less than 173 degrees and 32 minutes! This would look like a reversion of poles, or a reversion of influences. Again: were this the actual fact, the dipping needle placed at the magnetic pole, would not dip perpendicularly, but incline towards the terrestrial pole. It is not profitable to dwell upon the confutation of absurdities so plain as this, as it is manifest from first thought, that the hypothesis of two antipodal poles of equal intensity must place the line of no variation on that meridian which not only intersects the magnetic poles, but the poles of the earth; and the present position of this line would, therefore, according to your present hypothetical location of the poles, be such as to carry it through Lake Winnipeg, the Territory of Missouri, the western part of Arkansas, western border of the Gulf of Mexico, and into the Pacific near Acapulco; thence to the south pole, and finally through the Indian Ocean, Island of Ceylon, Indoostan, Chinese Tartary, Siberia, and thus on to the arctic pole.

You state in your memorial that you have with great care and labor constructed two series of tables, by which the angles which the magnetic axis makes with the geographical meridians, and those which the circle of no variation as well as the other magnetic meridians, make with the terrestrial meridians, on all points of the globe, may be at once determined; and your friend Dr. Dwight, in speaking of the tables, makes the highly complimentary observation, that, "To do all this, and so to do it that the results should always be attended with mathematical certainty, required very high powers of comprehension, analysis, and construction." Now, sir, let me observe that it requires but a moderate share of mathematical knowledge, to know that tables already exist exactly adapted to your purpose, and that any one capable of solving a spherical triangle, can "at once determine" any of the angles to which you have reference, without any "great labor and care," and without putting into requisition any "very high powers of comprehension, analysis, or construction."

Dr. Dwight in page 6th of the report, speaking of the magnetized circular plate, observes: "On moving the compass round the circumference of the plate, the needle declined towards the nearest magnetic pole at an angle proportional to its distance." This being the case, the needle, of course, must lie parallel with the magnetic axis when it arrives at the magnetic equator; for being there equidistant from the magnetic poles, will be equally influenced by both, and therefore, have no inclination towards one of them more than towards the other. Yet the Doctor, in the next sentence, says: "At the magnetic equator the needle lay parallel, not with the magnetic axis, but with the polar axis or axis of the plate." The extremities of the magnetic equator on the plate, are 46 degrees and 56 minutes nearer one extremity of the axis of the plate than the other; yet according to *one* assertion of Dr. Dwight, the needle in these places lay parallel with the axis of the plate, and of course pointed nearer one magnetic pole than the other by this same angle of 46 degrees and 56 minutes, although equidistant from them. According to *another* assertion of the Doctor, the needle declined towards the nearest magnetic pole at an angle proportional to the distance. These two assertions are completely at variance. Again: The Doctor says: "On moving the needle from one end of the magnetic equator to the other, the needle was every where parallel with the axis of the the plate." In the next sentence but one, he says, "On passing the needle from one magnetic pole to the other, its axis was in the line of the magnetic axis, and its poles pointed each to the dissimilar magnetic pole." The magnetic equator and magnetic axis intersect at the centre of the plate at right angles; the polar axis also passes through the centre of the plate where it intersects the magnetic axis at an angle of 28 degrees and 28 minutes. Therefore, according to the Doctor's logic, the needle, at the centre of the plate, being both in the magnetic equator and magnetic axis, has the very wonderful faculty of lying in two different positions at the same time! viz: in a line with the magnetic axis, and also in a line with the axis of the plate. To bring about such an extraordinary result, would be a far more difficult task than the construction of your tables, and require the aid of something more than your "very high powers of comprehension."

You state, in your memorial, that the plate has a line of no variaton.—If it has, it must, as it is a magnetic meridian, intersect the magnetic equator. But Dr. Dwight informs us in his letter, (and so said you in your lecture last summer, if my memory serves me,) that on either side of the magnetic equator, within the plate, one pole of the needle was drawn towards the nearest magnetic pole, or had every where a variation. The line of no variation cannot be the negative axis, for Dr. Dwight tells us that "On passing the needle from one magnetic pole to the other, its axis was in the line of the magnetic axis, and its poles pointed each to the dissimilar magnetic pole." Where, then, I would ask, is the line of no variation on the plate? To find its location and reconcile it with Dr. Dwight's description, would be far from on ordinary task.

Dr. Dwight informs us, (page 6, report,) that the dip was uniformly equal to the magnetic latitude, or to the distance from the magnetic equator measured on a magnetic meridian. I quote this to show, that according to your theory, the dip and the magnetic latitude are one and the same thing, or are always equal. It will be important to remember this, as I shall make allusion to it in examining the calculations of latitude, longitude, &c., by the dip only. Dr. Dwight also informs us (page 11, report) that with the correct dip as a datum, and that only, you are able, by your Geometer, to determine the longitude, latitude, and other important particulars. Sir, this is truly wonderful! And if you are really able to perform these operations, by the aid of your theory, your tables, and your Geometer, or by the aid of any other theory, tables, and instruments, ever invented by man, you must be possessed of extraordinary powers; for there is not a mathematician in existence but would declare them utterly incapable of solution. To determine a triangle with no other data than two sides and no angle, (and that is what your problems amount to,) is beyond the power of any calculus known to mathematicians, or that ever will be known to them. Such a problem is completely indeterminate—capable of an unlimited number of solutions. The arts of legerdemain and hocus-pocus are said to have performed wonders, and invocations to Abracadabra to have taught the secret thoughts of others, however remote; whether you made use of any such process or charm to solve your problems, so completely out of reach of human science, will probably forever remain best known to yourself. Permit me, however, to conjecture how you *may* have arrived at the solutions of such insolvable problems, and with such wonderful exactness as your solutions of the given examples show. The variation and dip have not been observed, recorded in books, and generally known, at so many different places on the globe, but that a good memory might be able to commit and retain them,—or at least, such as one would be most likely to be questioned about; and the ambiguity that would sometimes result from the fact that several places have the same dip or same variation, would be remedied, in a great degree, by the general data which you require; such, for instance, as whether the place be east or west of the line of no variation, whether it be north or south of the magnetic equator, and also whether it be near the arctic or antarctic semicircle of no variation. When the globe is thus cut up into sections of general data, it would be very rare to find two places in any one section, where the recorded dip or recorded variation would be exactly the same. Hence, in adopting this course, you are likely to avoid insuperable difficulties, and save the credit of your Geometer.

I will now proceed to the examination of some of the examples given in the 13th, 14th, 15th and 16th pages of the report, which, it must be acknowledged you have solved with a wonderful degree of exactness in point of fact, although the results are altogether inconsistent with your theory. How this could have been effected, will forever remain best known to yourself. It is beyond the comprehension of others.

The first example in substance is:—In London, in 1818, the dip was observed by Capt. Kater, to be 79 degrees and 34 minutes. Required the variation from this datum.

In solving this question by the Geometer, your result varies only about *a second and a half* from that of Capt. Kater by an astronomical observation. This is a degree of exactness which I am persuaded no other instrument would be capable of effecting; the variation by Capt. Kater's observation being 24 degrees, 30 minutes; and by your Geometer, 24 degrees, 30 minutes, 1 second, and 25 thirds! How you arrived at this result, so exact in point of fact, yet so completely at variance with your theory, I shall not inquire, as its irreconcilableness with the theory may be shewn without that trouble. We learn from your memorial, and also from the report, that the dip is invariably equal to the magnetic latitude; London must therefore have been, in 1818, in the magnetic latitude of 70 degrees and 34 minutes, or only 19 degrees and 26 minutes from the magnetic pole. The magnetic pole, by your theory, was, in 1818, in 86 degrees west longitude, and in the unchangeable lattitude of 66 degrees and 32 minutes north. London is in longitude 0, and latitude 51 degrees and 31 minutes north. Now by constituting a triangle, having given the arch of the meridian between London and the terrestrial pole, for one side, the distance between the magnetic and the terrestrial poles for the second side, and the difference of longitude between London and the magnetic pole for the included angle, we find London to be about 46 degrees from the magnetic pole, instead of only 19 degrees and 26 minutes; and its dip should, therefore, by your theory, be only 44 degrees. From the solution of the above triangle the direction of the magnetic meridian passing through London is north about 71 degrees west, and therefore, if the needle lie in that line, which, I presume, your theory supposes, the variation at London would be 71 degrees west, instead of only 24 degrees 30 minutes. Now, in order to bring about a reconciliation between your theory and the actual dip of London, you must either remove that city 1600 miles to the W. N. W. of its present location, or your magnetic pole as far the other way. With respect to the variation, the discrepancy between the theory and fact is no less glaring.

Similar absurdities are detected in the other examples, even without the trouble of calculation; but it is not profitable to swell this letter with their exposure. I will merely instance one or two of them, which are so glaring that they may be seen even by those who have no knowledge of spherics.—On page 14 of the report, the dip of Valparaiso is given, 38 degrees 46 minutes south; the magnetic south latitude of that place, is therefore, by the theory, greater than the terrestrial south lattitude, which would throw the magnetic equator, in that longitude, to the north of the terrestrial equator, and the magnetic pole over into the north of Asia, both of which would greatly contradict your theory! Again: According to the dip and latitude of Valparaiso it must be almost 6 degrees further from the magnetic than from the terrestrial north pole, when a mere glance at a map will show that it ought to be about 20 degrees nearer thereto. On page 16th, the dip at Washington city is put down at 71 degrees 13 minutes, and its latitude at 38 degrees 53 minutes. From this dip, Washington must be only 18 degrees 47 minutes from the magnetic pole, reckoned on a magnetic meridian; but from its latitude it is 27 degrees 39 minutes from the *latitude* of the magnetic pole, reckoned on a terrestrial meridian, and further still must it be from the magnetic pole, as there is a difference of longitude.

Sir, if you have made any important discoveries in the science of magnetism, and whether you have or not, is better known to yourself than to me, it is your misfortune as well as the country's, that they were not given to the public unconnected with such a tissue of absurdities as are contained in your memorial and in the letter of Dr. Dwight; unfortunate for you, as the result must forever blast your reputation as a man of science; and for the country, as it so narrowly and by mere accident escaped the imposture.

I am now through with the examination of your "Theory of Terrestrial Magnetism." I have handled the subject in the plainest manner I was capable of. A more scientific mode might have been adopted, but it would have been less interesting and have carried less conviction. It has been my endeavor to treat the subject fairly, exposing its merits or demerits as I found them, without regard to the wishes or predilections of its friends or foes. A regard for true science demanded this from my hands, and I have rendered a cheerful compliance. I conclude with the wholesome advice that you speedily publish such a statement as will carry full conviction that you were not aware of the absurdities of your theory, but that you verily believed it a true representation of nature; for, as the subject now stands, a suspicion of fraud, I fear, will rest upon your shoulders.

Yours, &c.,

EDWARD GIDDINS.

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